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Psychological Bulletin

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CONTENTS

Special Articles:

Mental Work: ARTHUR GILBERT BILLS, 505. *Educational Psychology—Some General Aspects of Learning and Teaching:* RALPH B. SPENCE and RUTH DUNCAN, 533.

Communication—A Correction of Authorship: LINUS W. KLINE, 566.

Notes and News: 567.

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THE
PSYCHOLOGICAL BULLETIN

MENTAL WORK[1]

BY ARTHUR GILBERT BILLS
The University of Chicago

I. Theoretical Aspects of Work and Fatigue

(a) *What Is Mental Work?*

The problem of defining mental work in such a way as to differentiate clearly between it and so-called *physical* work, raises certain questions. To what extent does mental activity involve the musculature? Can we say with Thorndike that mental work is predominantly work of the *connecting system*, or must we admit with Watson, that mental work is distinguished from physical work only by the *kind* of musculature involved? This problem is still in the theoretical stage. It is evident that motor psychology, or the hypothesis that muscular activity is involved in practically all thinking, is as popular as ever. Witness Langfeld's (32) recent address before the American Psychological Association, and Washburn's article in the *Psychologies of 1930*. Recently considerable experimental evidence has been brought forth relative to the problem. Golla (21) reports an investigation to determine the relation of increase of muscle tonus to prolonged mental effort. He found the same pattern of tonus change in both leg and arm muscles—an immediate rise occurring during the early stages of work, a period of maximum tension, and then a gradual return to the normal level, which preceded the onset of objective fatigue and often occurred while efficiency of output was still rising. When the task involved a progressive increase in difficulty of the work, the form of the tonus

[1] The present review covers the period from 1929 to 1931. The last previous review under this title appeared in the Psychol. Bull., 1929, 26, pp. 499–525.

curve was unaltered, *i.e.*, there was no prolongation of the high level. Stroud (**56**) studied tension changes in the active hand during stylus maze learning, by means of a specially devised pressure stylus. Subjects showed varying degrees of tension, but all displayed an appreciable amount. He found that tension tended to correlate with the difficulty of the problem, and when increased tension was voluntarily induced in the inactive hand, by pulling against a fourteen-pound weight, rate of learning was increased. Duffy (**11**) reports varying degrees of pressure on a bulb held in the unused hand of subjects engaged in a continuous discriminating reaction. Jacobson (**28**) has recently obtained action currents from many specific muscles during mental work, showing not only that thinking is normally accompanied by widespread tensions, but that it is impossible for subjects to relax while thinking. White (**68**), in studying the relation of the electrical resistance changes registered by the galvanometer to muscular tension, ran one series of measures on subjects engaged in silent multiplication. He found that the reduction in resistance caused by the mental work was even greater than that from localized muscular contraction.

On the basis of these and like findings we are justified in assuming that mental work *does* involve considerable muscular effort. Lashley, however, as a result of experiments performed by himself and his students on the relation of thinking to muscular activity of the vocal and visual mechanisms, takes the position that whatever contractions occur are overflow phenomena, and bear no causative relation to the direction of the thought process. His attitude would be that some muscular activity may *accompany* thinking; but that thinking is not a muscular activity, as the motor psychologists insist. A recent investigation on eye movements performed by Stoy (**57**) in which he measured the number and extent of ocular movements performed while mentally solving spacial and non-spacial problems, shows a greater frequency and amplitude of muscular contraction for the spacial problems. The issue is by no means settled, and merits a thorough study of the capacity for thought with experimentally induced paralysis of the relevant musculature. Meanwhile, we are compelled to recognize that at least a part of the fatigue of mental work is due to the accompanying muscular effort involved, even though we hesitate to define thinking as a muscular process.

(b) *What Is Mental Fatigue?*

The attack on this problem has been carried forward in two principal directions. (1) The recent study of nerve physiology has

yielded important results toward a clearer understanding of the nature of refractory phase in nerve, and has given an experimental basis for the concept of cumulative refraction, which, though postulated by Verworn (**62**) and adopted by psychologists to explain the ultimate breakdown in repetitive mental work, has lacked physiological verification hitherto. Field and Brücke (**15**) report that, under prolonged stimulation, the refractory phase of a nerve becomes longer. Forbes and Rice (**16**) note that when high frequency stimuli were applied to a peripheral nerve, and action currents recorded, the successive responses gradually became smaller.

(2) Several recent studies of continuous mental reactions, in which no opportunity was given for rest between responses, show effects closely resembling those of refractory phase, and cumulative refraction. Telford (**58**) points out that if a period of decreased excitability follows voluntary response, it should show itself as an increase in the time or decrease in the accuracy of responses occurring rapidly enough to fall within the successive recovery periods, or as a tendency not to repeat a given act for a time. He therefore performed an experiment to determine the effect of the length of interval between stimuli (1) on simple reaction time, (2) on accuracy of judgment, and (3) on tendencies to repeat in simple associative responses. The first question was answered by recording serial reaction times to simple auditory stimuli, presented at speeds varying from ½, 1, 2 to 4 seconds apart. The results on 29 adult subjects show that the shortest period gives the longest reaction times, around 3356; that the optimal time is from 1 to 2 seconds, and any further increase in time beyond this between stimuli again lengthens the reaction time. These results parallel the refractory period of more elementary systems, in which a subnormal phase is followed by a supranormal phase and then a return to normal excitability. The second question was answered by determining the accuracy of 25 subjects in judging linear magnitudes as influenced by the length of interval between judgments. Intervals of ½, 1, 2 and 4 seconds were used. Each subject made 325 judgments. Accuracy of judgment was much greater for the 1 second than for the ½ second interval, but decreased with any further increase in interval. Telford's third experiment, on the avoidance of repetitions, parallels Thorndike's study reported in our mental work article for 1929. Nonsense syllables, consisting of a vowel between two consonants, were read at varying rates. The subjects were instructed to write, for each stimulus read, some number between 1 and 10. The results show a tendency to avoid repeating the same number in proportion to the

shortness of the interval between stimuli. The intervals used were 1, 2, 4, 8, and 16 seconds. The 31 graduate student subjects showed an increase from 3.72 per cent repetitions at the 1-second, to 11.70 per cent repetitions at the 16-second interval. Within the limits investigated there was no reversal in the trend.

Bills (3) found evidence for a prolonged refractory period in repetitive homogeneous mental tasks, occurring about every twenty seconds on the average, although differing considerably from individual to individual and from task to task. Over sixty advanced students were tested repeatedly. The mental work consisted of color naming, code substitution, alternate addition and subtraction, naming opposites and voluntary perspective reversal, all performed at top speed. The responses were given with an average speed of more than one a second. In spite of the refractory periods or blocks which occurred three or more times a minute and lasted from two to four seconds each, there was no large general decrement in output over a period of an hour. The assumption therefore seems justified that the blocks served as short enforced rest periods during which the subjects recuperated enough to continue without diminished efficiency. The guess might be hazarded that these periodically lengthened cumulative refractory periods constitute a biological protective device automatically insuring against the ultimate exhaustion and complete breakdown which occurs in muscular operations, like ergograph work. This, then, would constitute a fundamental difference between the onset of fatigue in physical and in mental work.

M. Robinson (49) criticizes the entire concept of refractory phase as applied to mental fatigue, even in repetitive operations, on the basis of Lashley's neurophysiological discoveries. She argues that since mental operations are probably never limited to the functioning of isolated arcs, and since, according to Lashley, each repetition of a uniform task probably involves the functioning of quite different neural elements, it is doubtful whether cumulative refraction could ever develop. She therefore offers a substitute hypothesis to explain blocks based on her own introspections and those of others engaged in repetitive mental operations. This assumes a struggle for expression among many rival mental tendencies which are simultaneously aroused with the given tendency. Periodically, when the resistance to the expression of the rival tendencies is weakened, they are set off, and the given tendency is inhibited. She argues that, in this way only can the fact be explained that, during blocks, the subject's mind is not a blank but is engaged upon irrelevant thoughts. These argu-

ments might be met, by assuming that refractory phase can occur when large groups of neural elements are only partially excited, and that whenever an operation is repeated constantly, there is at least a partial excitation of common elements. That the subject thinks about other things, during the block, is not strange, for we should expect that, when the neural elements in question are temporarily exhausted, the threshold of excitation for other unused neural elements would be that much lower in proportion.

The seeming urge toward variation of activity, in the form of shift in mode of attack in a continuous task, has been observed by different investigators in both animal and human problems. However, their interpretations remain on the descriptive level and it is not evident that any new explanatory principle is involved. Lewin (38), whose point of approach is Configurational, points out that as soon as a task becomes so repetitive that the subject feels that he is simply marking time without achieving any particular goal, he tends to introduce variations in his mode of operation. He tends to create objectives. When all possible variations have been exhausted, the subject goes to pieces in the task. This seems to be an outcropping of the same tendency-to-avoid-repetition which Thorndike and Telford interpreted in terms of refractory phase. Striking instances of it have been pointed out in recent animal work. Maier (42) quotes experimental results of Lashley with rats in a simple maze. He found that, if 25 out of 52 rats took the correct path in a maze in which only a right or left turn was possible, only 8 of these 25 rats took the correct path on the next run. This looks like a definite tendency to avoid repetition. Maier himself, measuring the attention of rats in a discrimination habit, found that more errors were made on the average in the second or third trial in which the position of the stimulus light remained constant, than immediately after a change in its position. Obviously, the errors could not be due to incompleteness of learning, and he therefore concludes that they depended on the absence of change and the resulting inattention to the problem. Inattention is merely a functional term for what is probably at bottom a refraction phenomenon.

II. The Curve of Work

1. *Methods of Practice Equilibration*

Theoretically, the curve of work expresses variations in efficiency in the performance of perfected habits. It is therefore implied that practice in the function tested has already reached a practice

asymptote before the variations in its efficiency of functioning are studied. However, in practical studies of the work curve, practice has seldom been completely eliminated, and therefore various methods or techniques must be devised by means of which its effects can be averaged out of the final results. These techniques have been summarized by Robinson (48). They are:

(1) *The Method of Completed Practice,* which consists in bringing the subjects to a point of nearly perfect practice before the work experiment is started. This is seldom achieved in fact.

(2) *The Method of Predicted Learning,* which assumes that practice has been carried far enough so that the amount of additional practice effect from a given amount of additional performance can be predicted and discounted.

(3) *The Method of the Control Group,* which assumes that there are a large enough number of subjects so that two groups, an experimental group which is subjected to the experimental conditions and a control group which is not, can be considered as of equal ability, on the average. Any difference in their group performance is then attributed to the effect of the experimental condition.

(4) *The Methods of Counterbalanced Order,* in which the order of taking conditions is so arranged that practice is assumed to be equally divided between the two conditions so that neither one receives a greater amount *on the average.* This method takes several forms:

(a) The *ABBA order,* in which one condition receives the first and fourth, while the other receives the second and third positions. Assuming a linear practice function, then each condition receives the same total amount of practice. But practice is never linear.

(b) The *irregular order,* A,B,A,B,B,A,B,A, etc., which is used in cases where it would be advantageous to present the subjects from anticipating the order of conditions.

(c) The *simple A,B,A,B order,* where the precautions of (a) and (b) are unnecessary.

(5) *The Group and Counterbalanced Methods Combined.* The subjects are divided into as many groups as there are conditions of the experiment. Each group is then put through the conditions in a different order. When the results of all groups are averaged vertically, the various conditions are equally affected by practice on the average.

Certain pitfalls exist in these latter methods. The different groups may not all be equally affected by practice. Moreover, when the subject passes, say, from condition A to condition B, this

may be quite a different thing from passing from B to A. If the groups are sufficiently large, these objections become less significant.

2. *A Mental Work Formula*

In the interest of developing a psychodynamics to parallel physical dynamics, on the assumption that the energy of "mental effort" is distinct from physical energy, Weber (64) has attempted to develop a mental work formula. Mental work is by him used to refer to the amount of mental effort, or strain, subjectively appreciated, which is put into a given objective product. The physical formula for work involves three variables, *i.e.*, mass moved (M), time consumed (T), and distance traversed (D). The capacity of a moving mass to do work is then given by the formula $\frac{1}{2}M \times V^2$. That the formula for mental work differs from this physical formula was demonstrated by an experiment in which four subjects were required to produce subjectively equal muscular efforts by horizontal movements of the forearm. Three resistances, 140, 176, and 235 grams, were used; a total of 1,440 movements were recorded, each yielding a given Mass in grams, a Distance in centimeters, and a Time in sigma. To these data three formulæ were applied, to determine which would give the best fit or best express the equivalence of two efforts. It was found that the formula $\frac{1}{2}M \cdot \sqrt{V}$ gave the best results. Weber concludes that the basis for the difference between this and the physical formula is chiefly in the time factor. High velocities are not economical in physical work; but, in mental work, concentrated is more economical than attenuated effort, due to the factor of *voluntary attention* which he considers the essence of mental effort.

3. *Work Curve Phenomena*

The general contours of the typical curve of work have been fairly well established. The problem has now shifted to one of determining the degree of deviation from this typical form caused by a variety of factors. Wyatt (71) has suggested that it is possible to establish, for any given individual, a measure of his maximum capacity under the most favorable standard conditions. This is arbitrarily determined by his highest rate of working for a period of five minutes. With this absolute standard, a measure could be secured of the influence of any single, or combined group of factors detrimental to his productive activity, by getting the difference between this maximum and the average achievement throughout a spell of work. Applying this technique, Wyatt found, among other things,

that one worker might be almost twice as susceptible as another to unfavorable elements associated with conditions of work. Quick workers deviated more than slower from their maxima; and the difference between maxima and average rates of working might vary in different processes from 12 to 38 per cent.

How is the shape of the work curve affected by the nature or quality of the mental effort required? Most studies deal with relatively simple repetitive tasks. Netschajeff (44) has investigated the work curve of creative work in order to compare it with Kraepelin's work curve, and to determine the relation of the curve of creative capacity to the phenomenon of inspiration, which is the highest point in creative activity. His criterion of creative activity is power of imagination, or ability to manipulate impressions aroused by stimuli. Four different grades of imagination are indicated. He plotted curves showing the changing power of imagination during two hours of work. The curves show an early period of rise, to a maximum peak, followed by a later decline. The peak constitutes the point of inspiration. Subjectively, the initial rising period is accompanied by unpleasant strain, the attainment of the maximum by high excitement which is at first pleasant, later unpleasant, and the declining period results in a feeling of being set free.

A pioneer study of the work curve in animals is that of Sharp (50). His study on rats was an attempt to determine the effect of repeated trials on maze performance, and of the removal of the food incentive. The 181 rats were brought to a degree of practice such that 4 out of 5 runs were correct. They were then divided into 10 groups, 5 of which ran with, and 5 without the incentive. These were subdivided into five groups each, one being run one trial per day for 40 trials; a second, two trials per day for 40 trials; a third, four trials per day for 52 trials; a fourth, 12 trials per day for 60 trials; a fifth ran continuously for one hour, on six succesive days. The results show that repeated trials caused disintegrative effects, the amount of which was proportional to the number of trials run per day. The phenomena of initial spurt, general decrement, and end spurt were all present. Repeated practice tended to lessen the susceptibility to disintegration. This might be regarded as demonstrating adaptation to fatigue. In the case of the rats whose incentive was removed, however, the average amount of disintegration varied inversely with the number of trials run per day. Since the end spurt never occurred in the initial day's trials, it is probably comparable to the human phenomenon, which never occurs unless the subject has some inkling that the end of the day's work is approaching.

In the experiment of Bills and Brown (4) on the effect of the quantitative set on the shape of the work curve, it was brought out that the curve of muscular tension closely paralleled the curve of output during the early stages of work, but that after that the level of the tension curve remained relatively stationary. They obtained the tension curves in the following way: The task consisted of adding pairs of digits arranged in continuous rows on a sheet of paper, 45 examples to the row. The answers were written by the subjects on the sheet immediately below the digits. Beneath the answer sheet, five thin sheets of paper with carbons between were placed, so that the degree of tension used in writing could be measured by the clearness of the carbon copies. It is interesting that Golla and Antonivitch (21) got similar tension curves from mental work by an entirely different technique. Their method was to register the knee-jerk and wrist reflexes by means of a highly magnifying optical lever system. They got the same type of curve for both arm and leg tension. An initial rise of tonus occurred at the inception of work, and it remained at a maximum for a brief period, with a gradual return to the original level. The tonus curve receded long before fatigue showed up in the output record. When the work was arranged to become progressively difficult, the form of the tonus curve remained unaltered, *i.e.*, there was no prolongation of the period of maximum tonus. Is it not safe to assume that the period of heightened tonus represents a corresponding period of concentrated mental effort or attention, and that when this initial attentional effort is later replaced by an automatization such that the task can be carried on without attention, the tonus drops accordingly?

Glaze (20) has plotted curves from his study of fasting which corroborate evidence from many sources to the effect that fatigue decrements in continuous work tend to be lessened as the subject becomes more practiced in the task. This might be called "adaptation to fatigue." Subject A worked for 36 consecutive days on the task of writing *ab* continuously. When the average work curve for the first 5 days is compared with that for the second 5, a decidedly smaller decrement appears in the latter curve. Numerically, it is only about 30 per cent as great as the former. Apparently the practice effect has ceased by the end of the tenth day, as shown by a comparison with the average decrement for the last 26 days. Of course, in many experiments, there is another factor working against this effect which tends to conceal it. The work curve of an unpracticed function is likely to show very little decrement, because improve-

ment in the performance during the day's work masks the fatigue effect. When practice has reached a high point, the fatigue effect in the curve is no longer masked, and the manifest decrement is likely to be greater on this account. I have frequently obtained greater decrements on the fifth day of work studies than on the first.

4. *Fluctuations*

The problem of minor fluctuations in the individual work curve has received both theoretical and experimental attention in the past. Kraepelin's students, notably Von Voss (9), by a special method of computing response frequency, got apparently wave-like fluctuations or *Schwankungen,* in the speed of responses in continuous addition occurring every 2 to 3 seconds. But Thorndike (60) showed plainly that these were a by-product of the method of computation and not true output fluctuations.

The much longer oscillations which Flügel obtained at intervals of one minute in continuous work on addition, have been described in our previous review. Although probably an artifact resulting from the fluctuation in motivation caused by giving a warning signal each minute, they nevertheless point to a tendency toward periodic capacity fluctuations. Spearman (53) has recently based an hypothesis upon this and similar findings, to the effect that mental energy is subject to oscillations, which differ for different individuals. In fact, he makes O, the symbol for oscillation, the third general factor in his hierarchy of mental factors, G, O and P.

Bills (3) in the experiment on blocking, already described, has given corroborative evidence for Spearman's hypothesis, while at the same time explaining the puzzling fact that the decrements in curves of mental work are much slower in appearing, and much less severe than decrements in curves of muscular work. The blocks which occurred in this study, on the average, every seventeen seconds, constituted short involuntary rest periods in which the subjects recuperated enough to avoid any more prolonged breakdown. Between the blocks, the responses tended to bunch toward a center, thus giving the effect of alternate waves of condensation and rarefaction in response frequency. Errors tended to occur just before the blocks. It is therefore probable that the blocks represent low points, or troughs, in the efficiency wave, thus accounting for errors, while the periods of rapid responding between blocks represent crests in the efficiency wave or bursts of energy. These fulfill Spearman's concept of power oscillations.

III. Subjective Fatigue

To what extent is subjective boredom or fatigue feeling a direct determinant of objective efficiency? The consensus of opinion based on past studies is that the two are correlated, but not very closely. The curve of satisfyingness suffers a much earlier and steeper decrement than the output curve. There is reason to suppose that if feelings of fatigue could be removed, objective output would show a lessened decrement. One way of finding out would be to hypnotize the subject and convince him that he felt no fatigue, and note the effect on output. Nicholson (see **67**) performed such an experiment in 1920, using Mosso's ergograph for the work. In his control condition, no suggestions were offered. In the experimental condition suggestions were given throughout the work periods. He determined their influence on the total amount of work done, and also the influence of changing from the trance state to the normal and *vice versa*. Nicholson reported a very definite increase in muscular efficiency as a result of the hypnotic suggestion. And on changing quickly from the normal state to the trance, he found a marked increase in the amount of work and it seemed "possible to produce such a work curve indefinitely." Williams (**67**) has pointed out some defects in Nicholson's technique which allowed for indeterminate rests, etc. He performed a similar experiment, in which the suggestions were well standardized, and the shifts between trance and normal working state were instantaneous. He found that there was some increase in the work done under the trance, which would indicate a reduction of objective fatigue. However, he did not find it possible to obliterate the effects of fatigue by merely entering the trance state.

Experiments such as these throw a very indirect light on the problem at issue. A really crucial experiment would be one in which the investigator first determined the average decrement in output from, say, an hour's mental work; then the average decrement in subjective satisfyingness. Then a suggestion-hypnosis series, or a waking suggestion series, could be run in which the subject was persuaded that he felt no fatigue. A record of his objective output would be kept, and also a record of changes in satisfyingness should be taken as before, so that any decrement would be recorded. It would then be possible to determine quantitatively the exact amount of difference in level of satisfyingness caused by the suggestion condition; and the exact amount of difference in objective decrement.

A comparison of these would show how much influence on output a given amount of subjective change in satisfyingness would produce.

That feelings of tiredness may cause a decrease in accuracy without affecting rate of work, is shown in an investigation of monotonous work by Pollock (**47**). He gives results on one subject only, who was required to guide a metal ball to the top of an inclined plane with holes and bunkers as traps en route. She worked for spells of eight hours per day. Accuracy fell off in the later hours, while speed was actually increasing. Perhaps a similar relationship is present in Lewin's (**38**) studies of "Psychic Satiation." He found that when subjects were compelled to repeatedly read the same poem, or draw very simple patterns continuously, the mental breaking point, or point of satiety, came very early, long before any decrement in rate of output appeared; but the subjects tended to compensate for it by varying the nature of their responses in all possible ways. Obviously, in some tasks, this would be equivalent to a decrease in accuracy or quality of performance.

Wyatt (**70**) concludes, from a study of curves of output obtained from industrial operatives engaged in various repetitive tasks, in conjunction with the introspective reports of these operatives, that the semi-automatic task produces the greatest subjective boredom. It is far less likely to occur in the case of tasks requiring close attention, or those which are so completely automatized as to require no attention at all. Susceptibility to monotony is an individual matter. Several studies have been directed toward an analysis of the individual differences which predispose certain persons toward boredom. These will be described under the topic of Individual Differences.

IV. Physiological Effects of Mental Work

1. *Effect on Nerves*

Gerard (**18**) has carried out a number of studies of the effects of repeated stimulation on the metabolism of nerves. He has corroborated Tashiro's pioneer work, demonstrating beyond question that CO_2 is given off in amounts which exceed the resting metabolism of the nerve. He has also demonstrated (**19**) that the heat production from nerve stimulation is delayed, and may last as long as ten minutes after activity ceases. With repeated stimulation, the amount of heat per stimulation falls off, showing a lessening of response strength. The studies of Field and Brücke, and Forbes, already mentioned, are relevant here.

2. *Effects on Muscles*

That some muscular fatigue should accrue from mental work is an inevitable deduction from the findings regarding increased muscle tonus in thinking. Refer to studies of Golla, Jacobson, White, etc., already described. As a matter of fact physiologists report phenomena in the curve of lactic acid concentration in exercised muscles and adjoining blood vessels, corresponding roughly to the typical phenomena of the Kraepelin curve of work. Thus, at the beginning of work there is a very rapid increase in the lactic acid content. Later this decreases somewhat, and remains lower for a long time, but ultimately increases to a point considerably above the early high level. The analogy to the initial spurt and the drop from it, then warming up, and finally the onset of the final breakdown in the output curve, is striking.

3. *Blood, Saliva, and Urine*

Blonsky (**6**) made a study of the saliva of 412 Moscow school children at the period just before breakfast. The children with the more alkaline saliva proved to be the more intelligent, as tested by the Burt revision of the Binet test. He then selected ten children, and gave them a series of tests in which they were required to concentrate their attention on the solution of difficult maze problems. The alkalinity of the saliva increased. A third series, performed with six children who were asked to day dream for ten minutes, showed a decrease in the alkalinity of the saliva; and a definite acidity appeared in three cases. Shock (**51**), studying the effect of changes in the hydrogen-ion concentration of the blood on the efficiency of subjects in performing continuous mental multiplication and solving anagrams, etc., found the level of efficiency practically unchanged by the normal rises and drops in the blood curve from day to day. He artificially produced hyperalkalinity in one subject by feeding large amounts of soda bicarbonate. No marked change occurred in efficiency of performance. Artificially induced hyperacidity produced by doses of ammonium chloride also failed to disturb performance level, though subjective feelings were considerably upset. The induced hyperacidity was in this case greater than is commonly supposed to result in death.

Shock's findings, which were very carefully compiled, throw some doubt upon the importance of the changes in blood condition resulting from fatigue, as far as any effect on performance level is concerned.

No attempts to elicit objective signs of nervous exhaustion, declares an editorial in *Lancet* (**1**), have met with great success.

There is evidence of diminished sugar tolerance, an increase of organic phosphorus in the urine, an increase in the amount of adrenalin, and a change in the gonadal hormones. Continued states of anxiety cause an increase in metabolic rate.

4. *Breathing and Pulse Rate*

Skaggs (52) made a comparative study of thoracic and abdominal breathing, and pulse waves from the right carotid artery, during mental work, during excited expectancy, and during relaxation. The mental work consisted of continuous mental multiplication, and perspective reversal. The state of attentional strain with emotional excitement was produced by having the subjects sit, expecting a strong electric shock. The results show that mental work produces the most shallow, and most rapid breathing, while excited expectancy gives the deepest and most variable. Pulse rate is least in mental relaxation; averaging about 40 per half minute for men, it is increased in mental multiplication to about 43.7; and still further increased by attentional strain, to about 45.8. The mean variations are in the neighborhood of 3.5.

V. Facilitation and Inhibition

The recent contributions of Pavlow to our knowledge of the phenomenon of inhibition are chiefly factual, and hence, offer no essentially new theory of the nature of the central neural process involved. McDougall (41) argues that Pavlow's results lend corroborative evidence to his drainage theory of inhibition. It is true that, by a very broad interpretation of the drainage theory, *e.g.*, to the effect that the neural process which will be set up by a given stimulus is a function of the condition of excitation of all other regions of the cortex, the theory could be brought into conformity with Pavlow's results and all other similar ones. As to Pavlow's theoretical *interpretation* of his results, the analogy to McDougall's theory is striking.

Pavlow speaks of impulses aroused by external stimuli being "diverted and made to follow another route," and being "conducted to those centers" which are already in a state of excitation. Obviously, however, Pavlow has not left the descriptive level in his formulations. When an attempt is made to give an explanatory neural basis for the drainage hypothesis, and express it in less vague terms, difficulties are immediately encountered. McDougall evokes the concept of "vicarious usage of neural energy," by which terms he probably refers to the theory of vicarious functioning of cerebral

areas, in defense of his position. But he errs in identifying the two, and in quoting Lashley, who accepts the concept of vicarious functioning, but rejects the drainage hypothesis.

There follows a discussion of the facilitative and inhibitive effects of various particular factors:

1. *Distractors*

Weber (65) used detracting stimuli of three kinds—interesting anecdotes, lively music, and pictures—on sixteen subjects engaged in computation, cancellation, learning nonsense syllables, solving picture puzzles, thought problems, answering simple questions, defining, and composing essays. Control and test performances were alternated. Different attitudes were evoked in the subjects, such as directing attention to the importance of the work, or trying to eliminate the distractor. He found that the distractor produced its effect by arousing associations or altering the subject's mood. The least decline in achievement occurred when the subject's efforts were positively directed toward the tasks, rather than toward trying to disregard the distractor. There were individual differences in susceptibility to distraction, ranging from an 8 per cent to a 57 per cent decrease in efficiency, though the different kinds of work gave a correlation of .80 in their susceptibility to interference.

Hovey (26) in his studies of the effects of visual and auditory distractions on level of performance on the Army Alpha, found no influence of type of personality on susceptibility to distraction.

Ford (17) made an elaborate study of the influence of distracting stimuli in two forms: a Klaxon automobile horn placed about two feet from the subject's ear, and a phonograph with loud speaker close to the subject's head, playing records of humorous monologue. He chose, as his type of work, problems involving the selection and addition of single digit numbers from a row of mixed letters and numerals, the sums to be written on a strip of paper. Example: G d 7 F c 8 N f E a W q, etc. The purpose of this was to prevent automatization so that attention could wander from the task. Ford considers that he has thereby corrected a defect of previous work on distraction. Record was kept of the pressure exerted by the subjects in writing; articulation was recorded pneumographically; and the vasomotor phenomena were registered by plethysmograph. Each sitting consisted of three periods: first, six problems worked in quiet, then six with distraction, then six with quiet again. The phonograph was tried in a preliminary experiment and abandoned. So the results

are based chiefly on the automobile horn as distractor. The results, which include 183 sittings from 41 subjects, show that there is a prolonged initial reaction following either a change from quiet to noise or *vice versa,* but that subsequent reactions are less affected. Ford attributes this to the automatization which sets in. By this, he means habituation to the distractor, not practice in the task. That cessation of distracting noise was itself a distraction is shown in the subjects' introspections.

It is an interesting fact that the last problem done under distraction is performed faster than any in the subsequent quiet period, though the latter had the benefit of more practice. This corroborates Morgan's finding that after the subject has recovered from the initial shock of the distraction period, his efficiency actually exceeds that of the quiet period. Morgan attributes this to the extra effort called forth, and points, as evidence, to the increased tension used. Now Ford's tension records show a strong tension increase when distraction is introduced, then a drop, and then a rise to a maximum, as distraction goes on. With cessation of noise, tension again falls off. This also agrees with Morgan, and we see no reason for Ford's interpretation of his results as contradicting Morgan. He attributes the initial high tension to the extra effort of attention put forth to counteract the distractor, yet the subsequent rise in tension is explained as due, not to extra effort, but to adaptation to the distractor. He bases his deduction on the fact that high tension and slow reaction time are correlated .55, but this is not true for the distraction period taken by itself. There is a tendency for the vasomotor records to show, at the beginning of distraction, an initial constriction followed by a gradual dilatation, but this is no index to effort.

An examination of individual results showed that fast workers tended to recover from the initial shock of distraction much faster than slow workers, regaining their normal level almost immediately.

Whether Morgan's or Ford's interpretation is the right one can only be settled by finding out whether a person, working under distraction for long periods of time, suffers more fatigue than under quiet. In answer to this, we have some evidence from Laird (30). First, he has reviewed thirty-five titles, showing effects of noise on mental and motor functions and on respiratory and cardiovascular functions. His findings on metabolic rate have been reviewed previously. Second, he reports a recent study of his own (31), in which four individuals, equipped with stomach balloons, were subjected to

ten minutes of noise, of 80 decibels strength, following a twenty minute quiet record. The noisy period was followed by ten minutes of quiet. The kymographic records of the stomach contractions show a decrease of 37 per cent in the number of contractions per minute caused by the noise. The subsequent quiet period shows an increase in rate and violence of contractions to an amount considerably greater than normal. As a fair sample of what is going on in the body during distraction, this is significant.

2. *Rhythms*

Düker (10) studied the effect of tempo on quality and speed of work in adding figures and making paper bows according to a pattern. Two conditions were used: in one, the subject worked according to his own rhythm; in the other, he followed a definite tempo set by a sound hammer. Three subjects worked ten minutes daily for a period of sixteen days. The results show that when the arbitrary rhythm fits the subject's natural rhythm it increases efficiency; otherwise it is detrimental. The rhythmic work takes less effort, and, in the author's words, saves "psychic" energy.

Rhythms are used in teaching typewriting; either phonograph music, or tapping is introduced, or the subject is encouraged to work with a rhythm of his own. Entwisle (14) reports a study of the last sort. Two groups, paired in mental ability, were taught with and without rhythm respectively. The non-rhythm group wrote faster, but with more errors, and their net scores were somewhat higher. It is possible that voluntarily selected rhythms are not beneficial in the very early stages of practice in a new habit.

3. *Motives and Incentives*

(a) *Theoretical.* Some pertinent theoretical considerations with regard to experimentation on incentives have been suggested by Leuba (36). Emphasizing the symbolic and derived nature of incentives in civilized life, he warns against the assumption of experimenters that any one single incentive operates in a given experimental situation, and suggests a technique for isolating the stimulating power of any one incentive. For example, to measure the effect of praise it is insufficient to compare the output of a praised group over a certain period with the output of a control group. It is necessary to find out how much subjects will exert themselves rather than do without the incentive. Leuba describes an experiment (37) in which he used a method similar to that for determining thresholds in psychophysics;

i.e., he increased the requirements which the subjects must meet before the reward would be granted, until no subject attained it; then decreased the requirements until everyone attained it. The measure of the stimulating value, then, is the average of the maximum outputs under the conditions of increasing and decreasing requirements. Leuba has distinguished between "incentive situation" and "incentive attitude." He points out that incentives are named by the various situations which will arouse the incentive attitude. The latter is described as consisting mainly of muscular tensions which facilitate any activity in progress.

(b) *Reward and punishment.* Previous studies have suggested that severe punishment may act as an incentive or as a disruptive influence, depending on the attitude of the subject, his emotional stability, etc. Vaughn and Diserens (**61**) have given evidence that punishment, in the form of an electric shock, may affect efficiency of reaction differently from learning. Eighteen subjects were given one trial in running a stylus maze under each of four degrees of punishment, from zero to very strong. Reaction time in getting out of blind alleys was quickened by punishment, but there was an increased number of entrances into blind alleys, and learning time was longer. The more complex the alley, the more likely punishment was to confuse them. Obviously punishment tends to motivate toward quick and vigorous avoidance reactions, but interferes with deliberative thought, and hence its effect on learning is equivocal.

As compared with knowledge of results, Hamilton (**23**) finds reward and punishment superior. Working on the effect of incentives on accuracy in making visual discriminations with a Galton bar, sixty women were asked to judge lengths of lines. For reward, a bell was rung when the correct length was given. For punishment, a bell was rung when either too large or too small a judgment was made. Hence the same stimulus became either a reward or punishment, depending on the situation. One group was given knowledge in addition to reward or punishment, being told whether their judgment was too long or too short. One group was given knowledge but not reward or punishment. One control group was given nothing. The control group made no progress at all in accuracy. The reward and punishment groups benefited equally, showing no difference between the two types of incentives. Knowledge alone gave less improvement than incentives. When knowledge was added to incentives, there was no improvement over just incentives alone. These results are puzzling. Ringing a bell is hardly a punishment, though the

author reports an emotional disturbance in the subjects, which evidently exceeded that produced by a mere verbal statement of "right" or "wrong." But the bell gave no cue to the *direction* of error, yet adding knowledge to the bell did not give any added improvement. This would incline us to believe that knowledge proved beneficial merely by warning of error, rather than by telling the direction of the error, and hence this study really brings out the relative effectiveness of a bell as a warning, as compared with mere word of mouth.

In Leuba's study on 35 eleven-year-old children, working on two-digit multiplication problems, in which chocolate bars constituted the incentive, an opportunity was given to compare simple and complex incentives. The simple incentive alone produced a 52 per cent increase in level of output. When rivalry, praise and social recognition were added, the gain was 65 per cent, or an additional 13 per cent. Boys were more benefited than girls, which is attributed to the lower motivation of boys toward school work, when no extra incentive is present. Why not attribute it to a sex difference in appetite?

Two studies on reward in animal maze learning, one by Sharp, one by Blodgett, give an opportunity for comparison of behavior with and without incentive. Sharp (**50**), after training his rats with food incentive, removed it, and ran them continuously without reward. A marked disintegration occurred, but with practice this tended to disappear, and an actual improvement occurred. However, this must not be interpreted as disproving the necessity of incentives in behavior. The escape motive is always present, and, in addition, there is evidence that the performance of the act was itself a motive. Blodgett (**5**) ran his rats without an incentive, and later introduced one. They made very slow progress until the reward was introduced, when they forged ahead rapidly, making more progress than the control group who had been rewarded throughout.

Some further evidence that subjects working under strong incentive may lose in excess fatigue some of their gain in speed is given by Ketzer (**29**). In a laboratory study of the effect of time-rate vs. piece-rate payment on the work curve, three subjects were tested over an eight-week period. The curves show that average accomplishment is higher for the piece-rate method, that greater fatigue decrement occurs, and there is greater variability, because the subjects tended to work in spurts.

4. *Social Facilitation*

This factor is tied up with many others difficult to analyze out, such as rivalry, distraction, etc. For example, consider the results of

Ichheiser (27) on the influence of a spectator on performance. He gave a group of subjects the task of putting together a block test under two conditions—when alone, and when observed by the experimenter. The conditions were arranged as follows: The subjects performed the task twice alone, on one day. The second day, they performed it once alone and once with the experimenter present. The results show an increase of 46 per cent in accuracy and speed combined, under the last condition. Now the subjects had the advantage of each other's social stimulation under the other conditions, but the addition of one *spectator* made a tremendous difference. Many interpretations could be given. Perhaps it was the prestige of the experimenter; perhaps it was because he watched instead of working; perhaps the subjects thought their performance would affect their grades, etc. Burri (7), on the contrary, got detrimental effects upon the recitation of learned material, as a result of spectators. The subjects had to recite material, which had been learned with only the experimenter present, under two conditions—one with an audience present, and the other with only the experimenter there. The audience proved definitely inhibitive, and it made no difference whether they did or did not give attention to the subject's recitation.

Dashiell (8) has demonstrated that, when working *alone*, subjects may really be stimulated by the presence of an imaginary group, or have social attitudes established if they realize that others are working in neighboring rooms simultaneously. He also made a comparative study of working alone; working with a co-working but non-competitive group; working in competition; and working under the close observation of spectators. Multiplication, analogies and serial free association were used. The co-working but non-competitive group had no influence. This is contrary to Allport's results which showed an increased speed with decreased quality. In both the rivalry and spectator conditions, speed was increased over the solitary condition but accuracy was less.

Ekdahl (12) finds, from a study of the influence of the presence or absence of the experimenter on a subject's reaction-time in free association, that the majority of subjects give more rapid responses when alone. Thus, 104 preferred the "alone" condition, while 61 preferred the presence of the experimenter. The stimuli were visually presented by an automatic mechanism. The subjects' introspections point to a certain amount of inhibitive selection caused by the experimenter's presence.

Balderston (2), in a study of the relative effectiveness of group

bonus methods, where competitive groups are rewarded in terms of the performance of the group, versus individual methods of stimulation, finds the group method superior for the majority. Very fast individuals are somewhat detrimentally affected by the realization that the group is holding them back.

5. *Attitude*

The studies bearing on the relation of attitudes to efficiency of performance are too numerous to list. We have already had occasion to refer to some of them. Thus Weber (**65**), in his study of distraction, found that susceptibility to distractors depends on the attitude taken toward them. If the subject sets himself to avoid them, he will be more affected than if he sets his attention on the task to be accomplished and ignores the distractor. Lewin (**39**) studied the influence of attitudes aroused by success or failure in simple tasks, on tossing rings and shooting at a target. The subject continues only so long as the goal motivates. He found that success raises the standard of the goal 32 per cent of the time, and never lowers it; while failure lowers it 27 per cent of the time and never raises it. Lewin attributes the effect to the fact that failure causes the *ideal* goal set up to lose its reality for the subject. Obviously persons differ greatly in their susceptibility to such effects.

Monotony constitutes an attitude toward certain kinds of tasks involving repetition. Different individuals have the predisposition to develop this attitude in different degrees. Thompson (**59**), in devising a test for this trait, found that intelligence had little influence upon it. He gave his subjects four different kinds of work, each divided into a uniform phase and a varied phase. The per cent decrement for the varied phase was subtracted from that for the uniform phase, thus giving a single score which expressed susceptibility to monotony. When these scores for four tests were correlated with the criterion, *i.e.,* independent ratings of judges and personal history, the coefficient was .71.

VI. Diurnal and Seasonal Variations

Stainer (**54**), in an experiment on 532 school children whose ages ranged from ten to sixteen, attempted to determine the period of the day when their efficiency was highest, and the number of weeks of continuous work which could advantageously be carried on without a recess. The subjects were required to do simple additions of 3 figures, for 5 minutes, at intervals of 45 minutes during the day.

The curve of work shows steady improvement during the first 3 hours, a drop toward midday, then a slight rise followed by a fall. He concludes that 3½ hours is the maximum session for efficient work for children of this age. It will be recalled that Gates got a midday drop also, with similar work and similar subjects, but there was a subsequent rise up to 3 o'clock. In Stainer's second study, he tested the children at a specific time on the same day of each successive week for 13 weeks. Efficiency does not decline during a period of this length. Maity (43) reports tests of reaction time and ability to withstand fatigue which show no evidence of diurnal variations in the performance levels of 4 subjects. Unfortunately it is impossible to check on his procedure in obtaining these results.

Niceforo (45) compares the weekly performance curve of English and Italian factories. The English curve commences at a low level on Monday, rises during Tuesday and Wednesday, drops Thursday, and recovers through Friday and Saturday. The Italian curve differs in that the Friday and Saturday recovery do not take place, but the curve sinks to a minimum on these days; and there is no Wednesday peak. Some difference in mental attitude probably accounts for the discrepancy, rather than a difference in fatiguability.

Hersey (25) found evidence for periodic cycles of emotional tone in men. In the 12 subjects studied, he found that the cycles varied with the individual, the average being 5 or 6 weeks long, the minimum 3 weeks and the maximum 8 weeks.

Stainer (55), in a recent study on school children, practiced them in addition at regular intervals for an entire year. The practice curves show plateaux consistently during December, January and February. The plateaux may be merely a function of the stage of learning, but they may express an actual seasonal variation in mental efficiency.

VII. Kind of Work

Interesting experiments are reported by Netschajeff (44) on the degree of creativeness of the work, by Eliasberg (13) and Lofsagk (40) in compatibility of simultaneously performed processes, and by Lehmann (34) on the efficiency of dynamic versus static work, which the reviewer must omit discussing in the interests of brevity.

VIII. Speed and Accuracy

Is it more economical to stress speed or accuracy in work? To what extent does it depend on the task? Windmöller (69) experimented with 45 fourteen-year-old boys and girls, requiring them to

work on different tasks, such as typesetting, cancellation, addition, copying figures, cutting out patterns, arranging cards and figures, etc. All tasks were performed under three different instructions: (a) as fast as possible; (b) as accurately at possible; and (c) both as fast and accurately as possible. The results varied with the complexity of the task. Emphasis on accuracy for simple tasks cuts down speed without changing accuracy. Complex tasks, requiring attention to details, benefit from accuracy instructions alone, and stressing speed is detrimental. But individuals differ greatly in their responsiveness to speed or accuracy instructions. It has been pointed out that Pollock (47) found that feelings of tiredness correspond with a decrement in accuracy, without having any noticeable effect on rate of work.

IX. Individual and Group Differences

(a) *Age*

How does age affect performance in mental work, as to fatiguability, persistence, etc.? Curves of fatiguability ought to be worked out, comparable to the curves for growth of intelligence, from ages as low as three years up to adulthood. Then the curve should be extended to old age. Weiss (66) reports test scores for several hundred adults, including unskilled workers, railroad guards and locomotive engineers, from ages twenty to sixty. Intellectual performance declined steadily from age thirty; sensory performances and motor dexterity remained constant to about forty-five or fifty, then declined sharply. Heidbreder (24) made a comparative study of multiple choice problem solving at different age levels from three to ten years and adulthood. Errors, reaction time, etc., were recorded. It was found that problem solving ability increases with age; the problem solving set developed, as well as the capacity to adequately plan and rationally justify procedures.

(b) *Sex*

Most comparative studies on the sexes are invalidated by uncontrolled extraneous variables, as relative interest, practice, etc. It is practically impossible to isolate the sex variable. Hence not too much reliability can be placed on the results of Pauli (46) even though he used 100 subjects, men and women students. Three hours' continuous addition with pencil and paper were given to one group, and the other was given mental addition of increasing sums. The women were from 6 to 10 per cent slower than the men, while the men showed

greater extremes of variability in score. They were more accurate in mental and less accurate in written addition. Lewin (**38**) found women to be definitely affected by the menstrual cycle, in rapidity of satiation in tasks voluntarily undertaken. There was less readiness to engage in tasks and they were abandoned sooner.

(c) *Emotional Type*

The problem of the influence of differences in temperamental type on mental performance, has interested several investigators. Washburn and others (**63**) correlated type with speed, accuracy, and variability in reaction time to auditory stimuli, and in mirror drawing. The subjects included 33 emotional and 31 calm persons, 35 irritable and 41 non-irritable, 28 timid and 34 fearless, 51 extraverts and 55 introverts. They were all women college students. The results show a tendency for extraverts to react more quickly to auditory stimuli; for emotional subjects to be more variable in reaction-time; and for timid subjects to make fewer errors in mirror drawing, and to react more slowly than fearless ones.

(d) *Motor Type*

In studying individual differences in susceptibility to blocking in repetitive work, Bills (**3**) found that slow workers, those whose habitual tempo is low, tend to block oftener, and have longer blocks than rapid workers. Stroud (**56**) in his investigation of muscular tension found differences in type of tension; some employed strong, steady tension; others strong but variable; others used weak, steady tension; and others weak but variable. He found strong steady tension correlated with the highest efficiency in maze running. Duffy (**11**) noticed type differences in her tension study, but got no correlation between high tension and efficiency. There is possibly a relation between the so-called hyperkinetic and hypokinetic types and this tendency to use high or low tension. Lehmann (**35**), studying ergographic records, for differences in motor type finds three forms: an energetic type, an impotent type, and a fatigue type with very short ergogram.

X. Rest and Recovery

The curve of recovery from muscular work of the ergograph type has been plotted by Manzer and was described in a previous review. The curve shows a rapid recuperation at first, followed by a negative acceleration. At least 80 per cent of the recovery took place in the first five minutes of rest. The curve of recovery from mental work should be worked out. The author of the present review attempted

such an experiment and discovered several obstacles. First, it is practically impossible to produce complete exhaustion from mental work, because the subjects cannot be prevented from blocking; and these blocks give frequent short rests, as already pointed out (3). Consequently it would be necessary to use a relative, not an absolute, zero for the origin of the recovery curve. Second, mental fatigue is peculiar in that a subject's limit of capacity at any given time is extremely variable in measurement. Our procedure was to have the subjects write a short letter sequence repeatedly to the speed of a metronome. The metronome was set at a speed which was as fast as the subject could possibly write for ten seconds. Starting at this rate, the subjects worked until they could no longer keep up. The speed was then reduced slightly, and they again proceeded until another reduction was necessary, et cetera. When the subjects were working at half their original speed (which occurred within fifteen minutes time on the average), they were stopped and immediately tested at top speed for ten seconds. Rest was then given and the different groups of subjects were retested at different intervals afterward, varying from ten seconds to five minutes after cessation of work. It was found that recovery was very rapid; that the curve was negatively accelerated; and that complete recovery had taken place within three minutes. But the absolute averages have large standard deviations. Further experiments are being made which may yield not only more reliable results, but also recovery curves for several different degrees of original fatigue.

The effects resulting from loss of sleep are applicable to the problem of rest, but lie beyond the scope of this review. An excellent recent investigation of the subject is that of Laslett (33).

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EDUCATIONAL PSYCHOLOGY—SOME GENERAL ASPECTS OF LEARNING AND TEACHING

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With the widening of the concept "education" there has been as an accompaniment the widening of the field of Educational Psychology. In place of the usual summary presuming to cover the whole field as has been the case for the past few years, there will be summaries on various aspects covering a period of three or four years.

It is not easy to find a satisfactory line of demarcation, however, and the term here used leaves much to be desired. Material in psychology might be placed on a scale ranging from that which is very general in its applications and implications (facts about individual differences, principles of learning, etc.) to that which is useful only in a particular type of situation (experiments on methods of teaching subtraction, or an analysis of errors in spelling). This distinction is difficult to use because many of the more specific studies if well done have implications with regard to the wider principles.

In general, however, we have left out that type of material which is covered so well by the members of the staff of the University of Chicago. We have also excluded material dealing with such topics as character, personality, mental hygiene, delinquency, special abilities and disabilities. The literature dealing with tests is also omitted.

On the other hand, there will be considerable overlapping between the material here included and other BULLETIN summaries, particularly those of McGeoch (**159, 161**), Bills (**13, 14**), and Diserens and Vaughn (**51**).

MOTIVATION

The material dealing with motivation will not be dealt with extensively here because the topic has already been covered in the January, 1931, number of the BULLETIN by Diserens and Vaughn (**51**). The bibliography includes 233 titles from both animal and human experiments.

The problem of motivation is interwoven with other factors so that it is difficult to separate out the element of motivation. Knowl-

edge of results, for example, probably tends to increase the active drive or drives under which an individual is operating and also tends to improve learning by increasing the identifiability of the correct responses. Some of the possible motivating factors discussed by Diserens and Vaughn (51) are suggestion, knowledge of results, rivalry and competition, praise and blame, effect of an audience. Many of these are complex combinations.

Maller (156) found that in general pupils tend to do more when working for themselves than when working for a group. There were wide individual differences, however. The constitution of the group also tends to modify the results. Carpenter and Fort (34) found a positive correlation between the number of visitors in classrooms and the activity of the children. Knowlton and Tilton (123), Wood and Freeman (264), and Charters (41) report improved results in achievement through the use of moving pictures and the radio. Barr (8) and Taylor (235) report studies dealing with the influence of the teacher. The factors back of the differences are not clear. Duffy (57) found no evidence of a definite relation between tension and degree of efficiency nor that excitability is correlated with variability in performance.

The function of extracurricular activities as motivators for regular curricular studies (a relatively minor point, but one often raised) does not seem to be very marked. Hindman (105), Hutchinson (116), Ryan (209), and Savage (211) have all made comparisons relating to participation in athletics. In general the differences between participants and nonparticipants are very small. Hindman reports that at Ohio State University athletes stay in school longer and get proportionally more degrees than the general student body. Chapin (39) found 300 active extracurricular activities at the University of Minnesota and the more active a student is in terms of the number of activities, the more intensive is likely to be his participation in them. His study shows no detriments to the active participants. Knox and Davis (134) found that participation in more than two activities tended to be detrimental for men but this did not hold true for women. Monroe's (172) findings were generally favorable for participants in activities. Constance (44) made a very interesting study of the achievement of students from different fraternities. Making allowance for differences in intelligence and high school preparation he still found large differences between scholastic achievements of the different groups.

Crawford (46) in an extensive study presents data to show the

value of scholarships as motivating factors. In general, however, the differences were not great. Smith (219) reports the effect of excessive emotional reaction in producing mental confusions in examinations. Herriot (103) reports that the most important attitudes in relation to school success seemed to be an evaluative attitude and a perseverative attitude.

Learning

Hunter (113) has an excellent discussion of various experimental studies on learning and Lashley (143), McGeoch (159, 160, 161), Bills (13, 14), and Shirley (216) have reviewed special aspects. Much of the most careful experimental work in this field has been done with animals but the interpretations are difficult because of several factors. The question as to the applicability to human problems of results derived from animals is hard to answer and varies with the problem studied. Lashley (142) feels that there is much evidence in favor of applicability of studies on the nervous system but Coghill (42) while holding the same belief admits that there may be changes in function with the development of the nervous system in the higher animals which makes comparison difficult.

Husband (115) compared adults and rats in maze learning and found human learning curves in general like curves for animals although always lower. Problems which complicate the interpretation of animal studies arise around the tendency to retrace (Warden and Cummings, 248) and the inability to control the hunger drive because of lack of adequate developmental norms (Stone, 227). This lack of control over incentives also complicated the interpretation of experiments in which S's are human.

Thorndike (239) has given some of the results of his recent experiments in learning. No brief account here could do justice to the mass of material presented in style as fascinating as a novel. Assuming always the limitations of the experimental samples he presents new evidence regarding the principles of learning. Some of these are:

The zero or practically zero effect of mere frequency of connections.

The principle of belonging when effect is zero tending to produce more learning but not much.

The failure of punishment to weaken connections.

The demonstration that with frequency constant, successful responses are strengthened even when S is not aware of success.

The demonstration that insights are often the products of habits.
The importance of identifiability and availability.
The explanation of thinking in terms of general laws of habit formation.

The treatment of other theoretical explanations is inadequate and disappointing.

A number of studies relating to the rôle of attitude or set or direction in learning have been made with important implications for teaching. Maier (**155**) studied the effect of various combinations of directions and experience in the solving of original problems. For some individuals he found that so-called "helps" were rather hindrances for learning. Carr (**35**) came to similar conclusions in his study. He also found that tuition by a manual method in learning maze A enabled the students to learn maze B more rapidly than students who learned A without guidance. One of the most interesting results of Carr's study was the fact that even with a short, concrete, and easily remembered rule, 8 of the 30 failed to succeed in a rational problem. Sengupta and Bose (**213**) got much greater success in picking out numbers when the S's definitely knew what to look for. Bills and Brown (**15**) investigated the effect of a quantitative set. The experiment covered only short work periods but the results indicated the influence of an "amount set" as contrasted with a "time set." As the amount of work to be done increases, more time per unit is required. Freeman (**69**) concludes from his study that the learning of complex material by repetition without favorable attitude is, to say the least, uneconomical. Yoshioka (**268**), working with rats in maze learning says, "A readiness to learn a particular maze habit seems to be a function of natural ability or insight or intelligence or whatever else it may be termed, something that originates in the rat and not of the frequency imposed on the rat *ex cathedra*."

Stephens (**224**) experimented with 56 college students in a laboratory situation in which each S tried to hit a target he could not see. Different consequences for success and failure were used. Stephens' results agree with some of those of Thorndike that subsequent conditions do not act merely as cues for decisions but affect the learning in some additional way. Hamilton (**95**) compared groups on the Galton bar using as consequences the ringing of a bell when correct (reward), ringing a bell when wrong (punishment), guess with punishment, being told when wrong together with punishment, and knowledge of results. There was no difference between reward and

punishment. Knowledge of results without attitudes was less effective. Mere practice gave no improvement. Sanderson (210) found a pattern intention had a variable effect in two types of situations.

Freiberg, Dallenbach, and Thorndike (71) tested the supposed tendency of a series ABCDE to change toward A...E, the middle members tending to drop out, by examining typesetting errors. Errors were not most frequent in middle of words nor in commonest words, which the authors interpret as a failure to confirm the supposed tendency. Cuff (48) interprets inconsistent values obtained for reading beyond the point of immediate recall in terms of a principle of use. Nathanson (179) reports the curing of habit difficulties by a method similar to that proposed by Dunlap, although the explanation is in different terms. Wakeham (245, 246) reports two tests of Dunlap's hypothesis without achieving satisfactory results.

McGeoch (160, 161) continues his excellent work in summarizing the studies dealing with memory. Meltzer (165) criticizes previous studies on the relation of feeling to memory because of their failure to measure accurately the various factors involved and because of the tendency to assume causation from degree of association. Krueger (138) studied overlearning and concluded that overlearning of at least 50 per cent is desirable and the value of overlearning is more pronounced for the longer intervals of recall. Spight (221) compared day and night intervals in relation to the distribution of practice and obtained superior results for the night intervals.

Students of Gates have attacked the problem of generalization through the medium of spelling. Thompson (237), Carroll (36), and Mendenhall (167) have found that there is no lack of generalizations in the spelling of elementary school children but it is a question of the kind of generalizations and the manner of application. The problem of generalizations is closely related to the problem of transfer as discussed by Orata (186). Some of Orata's statements are so naïve as to be ridiculous (Robinson, 205), but there is no doubt that this phase of learning needs extensive comprehensive research. But then, what phase doesn't? Yum (269), working on the principle of assimilation, got results which are suggestive for the problem of transfer.

Anderson (1) obtained better results for some functions when S's worked alone and for others when they worked in groups. Different levels were affected differently. Bills (13, 14) summarized a number of articles on mental work and in connection with monotony reported that it is more dependent upon attitude or set than upon the

type of work involved. Pollock (195), Thompson (236), and Wyatt (267) have studied the problem of monotony and boredom. Pollock found that concentration on speed tended to increase both speed and accuracy. Walther (247) was able to get from 30 per cent to 40 per cent improvement without speeding up by applications of testing and time study.

Crafts (45) studied whole, part, and combination methods in card sorting and found whole method best and pure part poorest. Darrow (49) has reviewed the evidence on the effects of drugs and reports that with some exception the evidence seems to show that alcohol tends to impair all levels of performance. In regard to tobacco, nonsmokers were found to be better students and athletes than smokers but this does not indicate causative relationship. Maurer and Tsai (157) found that rats which were deficient with respect to vitamin B were very much less able to learn than normal rats.

Moore (173) gave reasoning tests of three types to children and found that autistic fallacies were not detected until third grade. In adults significant failure on this type of material seems to be related to deterioration. Laycock (146) has endeavored to work out some of the implications of Spearman's neogenesis to school situations. Kreezer and Dallenbach (137) give some interesting results for the learning of the concept of " opposite " by children, ages five to seven–six. Brian and Goodenough (22) found that except between ages three to six, form is preferred to color as a basis of matching when either is possible. Bischler (16) has studied the development of ideas by analysis of reactions of adults to pictures and objects. Atkinson (3) as a result of his experiments suggests a differentiation between intelligence (general ability to react consistently to complex situations) and learning (ability to react to more restricted and specialized situations in such a way that a well integrated adaptive response of a mechanical type is rapidly perfected under continuous practice). In experimenting with animals Gengerelli (77), Hsiao (107), and Maier (154) all report evidence of insight or generalizing ability. Warden and Cummings (248) were unable to confirm the common belief that rats tend to learn mazes regressively.

Willoughby (260) compared digit symbol substitution and ability to recall code with S's over a wide age range and found the functions did not seem closely related. Age curves for two abilities were different. Glanville and Dallenbach (83) conclude that range of attention is always one (unity) and the study of number of contents

simultaneously experienced is a question of cognition. Elkine and Krasnopolsky (**60**), using an associative series, found marked retarding effects when common member was displaced near middle or end of the series. Doll (**52**) suggests a clinical method in the study of learning situations.

In an attempt to throw light on the constitution of abilities Robinson and Richardson-Robinson (**206**) had 81 subjects draw 40 lines for each of 10 standard lengths. When the results were correlated, r's varied from about .96 for self r down to .36 when 6 cm. and 33 cm. lengths were compared. Garrison (**72**) found low correlations in the study of speed activities and Estabrooks (**63**) found little relationship between various tests for suggestibility.

McGeoch (**159**) has summarized very thoroughly the studies on the acquisition of skill. In an experimental study McGeoch and Melton (**162**) found that under equal conditions, motor habits are not better retained than memorial materials. Brian and Goodenough (**22**) found that children four and a half years of age learned to toss rings bent when given a definite method and made to stick to it. Gesell and Thompson (**81**) in a study with identical twins throw light on the effectiveness of training at various stages of maturation. Gemelli (**76**) as a result of his studies suggests a classification of motor skills into four types. Jacobsen (**118**) reports an interesting technique which involves getting rid of last remnants of muscular tension and his studies are suggestive from the standpoint of psychology.

In general, as is to be expected, the theoretical discussions, while calling attention successfully to the defects of present systems, are weak on the constructive side. Part of the difficulty is in the terminology. Ballard (**4**) eulogizes Spearman as the discoverer of the "electrons" of psychology but others are not ready to subscribe to this claim. As far as psychology itself is concerned the attacks and counter attacks may have a certain salutory effect, but the confusion is often troublesome to educators who look to psychologists for help.

Lashley (**141, 142**) has continued his studies of the effect of destruction of various parts of the cerebrum upon learning ability. The results vary with the type of maze used but in general the tendency seems to be for learning ability to decrease in proportion to the cerebral area destroyed but to be independent of the location of the destroyed area. Lashley's experiments need verification on a larger number of cases but his results so consistently point in the same direction that the general conclusions seem sound. The results call for

the abandonment of the widely held concept of synaptic learning. In a study with Miss Ball (144) he has further succeeded in destroying the afferent and efferent spinal paths without disturbing accuracy in running the maze, indicating that there is some intraneural arrangement which produces an integrated series of movements in the absence of directive sensory cues.

Crozier (47), Forbes (68), and Hecht (96) review a wide variety of studies bearing on the understanding of the nervous system and its relation to behavior and in the Encyclopedia Britannica, Morgan (175) and Hempelmann (99) review animal behavior and comparative psychology. From an impatient point of view of one interested in human conduct the results may seem slight, yet the approach seems fruitful and as Lashley has pointed out, in the future there should be a more effective integration of neurology and psychology.

Gundlach (92) analyzes the differences of theories around four problems while Bode (18) reduces everything to one problem—the mind-body difficulty. Bode's positive contribution is unconvincing and disappointing as he has failed to consider some of the newer approaches which seem closely related to his development of his thesis. Weiss (253) in a second edition has added new material without finding it necessary to change his point of view. Köhler (136) has written a book on the Gestalt Theory for that portion of the public interested in newer developments in psychology and Petermann (192) has a more technical analysis of this school. Grant (87) uses with doubtful value the nomenclature of the Gestalt Theory in connection with the inability of college sophomores to understand certain concepts. Herrick (101) develops the mechanistic approach in a very broad way and Stephens (223) and Hull and Baernstein (111) have worked out machines which to a certain extent parallel learning situations.

Pratt (197) endeavors to restate faculty psychology for present day conditions and Swift (232) develops a concept of physiological tensions in order to explain better the problem of elimination of errors. Winsor (261) criticizes as misleading explanations which use the concept of "weakening." Williams (257) criticizes the conditioned reflex as a general explanatory principle. Hsiao's (108) question propounded in the title to his article is much easier to answer than his "behavior irradiation" is to understand. Piéron (193) gives a general exposition of mental functioning. Lorimer (152)

writes around the thesis that the evolving structure of intellectual activity is a function of the total growth of life.

Drever (56) has a brief summary of the concept of instinct but fails to mention the suggestions of certain writers that the term be abolished. Fletcher (67) thinks the problem can be solved via a Lamarckian hypothesis revived by some recent experiments with colon bacilli. Peck (189) has an excellent, nondogmatic presentation of the meaning of the term psychoanalysis, principally from the Freudian viewpoint.

Individual Differences

Many of the articles on this topic are primarily concerned with data from intelligence tests and have been covered in the bibliography on that topic.

Jennings (119, 120) has given a most excellent presentation of the reciprocal function of heredity and environment. Popenoe (196) has a good popular account of the facts about heredity written for parents and educators and Morgan (175) has summarized the problem of heredity in relation to psychological problems. Gesell (79) discusses maturation in the light of his studies with infants and believes it operates as a regulatory mechanism. Kuo (139) maintains that the concept of heredity is confusing and damaging for real scientific studies.

Sex differences are slight in comparison with the range of individual differences for each sex. Perry (191) in the analysis of her data finds that in certain aspects of geometry girls were superior to boys. Bassett (10) found that boys retained history slightly better than girls. Kaulfers (130) found that boys were consistently lower than girls in the study of Spanish and required about 10 per cent more intelligence to attain the same level in grades. These studies should be interpreted in light of Lentz's (148) questions about the relative validity of marks for boys and girls. While his study is not conclusive, he found that boys exceeded girls on the Stanford Achievement Test but were exceeded by girls in marks given by teachers. Miles and Terman (168) show that differences between sexes in quality of word association tend to disappear when training and experience are similar. Broom, Thompson, and Bouton (26) found that judges could determine sex correctly from handwriting in almost two cases out of three.

Gray and Bingham (88) find that whites tend to exceed negroes

on Seashore tests. Pratt (198) compares Chinese, Japanese, and Hawaiians in school achievement in Hawaii.

It is hard to see the value of some of the studies on sex and race differences. Sex is certainly not a significant index of individual differences. It might be better to put the emphasis upon adjustment to individual differences directly. In the case of racial and national studies there is unnecessary readiness to rush into print. There is a feeling not uncommon that to measure almost any group and report the results in terms of racial or national background is useful. Wood's (263) study on Otis S-A results in a Constantinople college based on approximately 25 cases for each national group is an extreme example. When one considers the complexity of the factors involved in interpreting such results, and the social consequences that may result, more reticence in publishing results might be beneficial.

Haefner (93) examined 1,144 children and found 6 per cent were left-handed, while 5 per cent were mixed and 1 per cent were classified doubtful. There is some slight evidence for a relationship between change and speech trouble. Wetmore and Estabrooks (254) found no relationship between left-handedness and psychoneurotic traits as measured by Laird's B-2 Scale. Oates (182) states that departure from unilateral functioning is definitely related to complications in the nervous system. Heinlein (98) found a variability in hand preference for a large percentage of the children studied.

The general shape of the learning curve from birth to sixty years has been presented by Thorndike (238) and corroborated by Jones, Conrad, and Horn (128). There would seem to be wide educational implications involved in this evidence, although it is interesting to note that almost twenty years ago Thorndike predicted the facts now verified by research.

Ability grouping is being subjected to considerable criticism which should prove helpful. There is no doubt that so-called "homogeneous" grouping was uncritically employed in a large number of cases. Many schoolmen seemed to assume that the mere formation of ability groups would solve their educational problems. In other cases the results were prejudiced by unfortunate attitudes which are so often associated with intelligence tests.

Rock (207) has made a very thorough study of 110 articles dealing with ability grouping. After summarizing the various features he concludes:

"The experimental studies of grouping failed to show that the pupils in homogeneous groups made greater gains than pupils of

equal ability who received instruction in heterogeneous groups. However, the experimental studies presented are not to be taken as evidence of the failure of ability grouping because not a single study was carried out under conditions that could be considered favorable. No regular provision was made for differentiation of teaching methods or curricula. In every case the period of the experiment was too short to permit adjustment of pupils and teachers to the new situation. The possibility that pupils grouped homogeneously made gains in traits other than the school subjects was not considered in these studies, and no attempt was made to measure these less tangible traits. The conclusion to be drawn from the experimental studies is that grouping, without further adaptation in regard to teaching methods and subject matter, does not result in greater accomplishment than under the heterogeneous class organization. There was practically unanimous agreement, among the teachers involved in the studies reported, that there was a marked improvement in the attitudes of pupils and in the general teaching situation resulting from the homogeneous organization. It is possible that this improvement alone would justify the practice of ability grouping, although it is not admitted that the advantages claimed for ability grouping cannot be attained under good conditions."

Lincoln (**150**) and Miller and Otto (**170**) have also summarized the results and report variable results. Sometimes the ablest pupils profit, sometimes they don't; sometimes the slow groups show the most gain. Lincoln and Wadleigh (**151**) report teacher opinion from a city which has been using grouping. From 58 per cent to 67 per cent of the replies were favorable and 17 per cent to 20 per cent were unfavorable. There was considerable disagreement on the attitudes produced. Purdom (**203**) in general reports against grouping although the experiments are not conclusive.

McGaughy (**158**) and Burr (**29**) have demonstrated that grouping usually does not reduce the range of ability with which the teacher has to deal. The uncriticalness with which grouping was accepted by educators is made more evident by the fact that the facts revealed by McGaughy could readily have been deduced from the known correlations between intelligence and achievement. Pyle and Snadden (**204**) also give concrete evidence of the overlapping between groups. Kelliher (**133**) has ably dealt with the problem of homogeneous grouping from the standpoint of educational philosophy but does not go on to work out the broader implications of her attack. Symonds (**233**) has sympathetically reviewed several of the recent

articles dealing with the problem of homogeneous grouping and concludes: "If pupils are to be instructed in groups, as promises to be the case for some time to come, certainly ability is one very important, if not the most important factor to be considered in forming these groups."

Kefauver (131) has analyzed a variety of factors as bases for forming ability grouping. Odell (183) reports an evaluation of different quotients which are being used but presents no direct experimental evidence. Heilman (97) in an analysis of factors determining achievement and grade location reports that 57 per cent of the variation in educational age is due to mental age but that in determining grade location M.A. is given little weight.

In general while considerable improvement has resulted during the last fifteen years, school adjustments to individual differences are still unsatisfactory. Measures of any kind with a reliability high enough to use in connection with individual guidance are scarce. Studies like those of Dearborn and Smith (50), Kefauver (132), and Madsen (153) show the necessity for extreme caution. Help is needed from educational leaders regarding the goals of education. Future practice in the treatment of individual differences should rest on carefully controlled experimental studies and on the continuous scrutiny of the underlying assumptions involved. Present evidence is inadequate to answer the problem.

Class Size

The experimental evidence on class size is particularly difficult to interpret because in the experiments which will be presented such factors as the method of teaching and individual attention for the students have not been taken into account or controlled in any definite way and the measurements are limited.

M. B. Jensen and D. W. Jensen (123) made a summary of the influence of class size in both elementary and secondary school subjects. Using the school marks as a basis for success, they found that in elementary grades pupils in classes of 45 to 50 or 55 pupils made as high test scores as those in smaller classes. In high school subjects, they found that English students made best progress in classes of 20 or under, the modern language students in classes from 20 to 30, and social science and mathematics students in classes over 30. M. B. Jensen (122) made a further study of algebra and English classes. He found that a significant difference in accomplishment as measured by both an Algebra Content Test and teachers' marks

appears. The students in small classes showed at the end of the semester an unquestioned superior accomplishment as compared with their peers in intelligence in large classes. In the English classes the same experimental conditions prevailed as in the Algebra classes except that the only measures of achievement available were teachers' semester marks. There was a marked superiority of small class instruction for boys and no difference between small and large class instruction for girls.

Holland (**106**) presents evidence on class size from a course in Educational Psychology. Subject matter, tests, and methods of instruction were kept the same for all eight sections of the course. These sections contained different numbers of students ranging from 112 to 34. At the end of the semester he found that the students in the large classes acquired no less of the course subject-matter than the students in the small classes. There seemed to be a tendency for the students to acquire the subject-matter according to their I.Q. ratings.

Hudelson (**110**) has presented extensive data which show that as far as the measurements used were concerned large classes are not inferior to small classes four times out of five.

Lecture Method vs. More Individualized Methods

The lecture method so prevalent in colleges and universities has been much criticized. Also experimenters have been busy comparing it with other more informal and individualized methods of teaching. Greene (**89**) has compared the amounts of learning which result when one group of college men hears a lecture while another group of nearly equivalent ability reads practically the same material for themselves. Immediate objective tests on the material show no significant differences between the two groups. There was a tendency for the students in the upper quartile as regards intelligence to make slightly higher scores after reading the material themselves than after a lecture presenting it, and the reverse was true of the students in the lowest quartile in intelligence. The lecture group took more notes than the reading section and on a retest after two weeks the lecture group made a better score than the reading group.

Tuttle and Douglass (**243**) experimented with a college class in educational psychology. They divided the students into two equivalent groups on the basis of intelligence test scores, reading ability scores, previous training in psychology, and knowledge of the subject-matter of the course as measured by an objective preëxamination on

the course. One of the sections was taught by the lecture method and was given two lectures and one quiz section a week. The other section met three hours a week but there were no lectures. The students were divided into committees of four which worked out problems corresponding to the main points being presented in the lecture section. The results as measured by the objective examination on the course were identical for the two methods in the case of students with no previous training in psychology. The students who had had previous training in psychology gained 24.8 per cent more by the "project-class" method than by the lecture method. The students seemed to like the "project-class" method.

The lecture method has been compared with individual study by Taylor (**243**). The experimental section was given no lectures or instruction, but reported to the reference library where they could study with the aid of the detailed syllabus given to both the sections. An instructor was available for consultation. In contrast to this the other section was given lectures using the syllabus as a basis. In the final examination the experimental section made slightly lower scores than those made by the lecture method section. The experimenter suggests that the experimental section was at a disadvantage because the points in the syllabus which formed the basis of the final examination had been specially brought out in the lecture section.

Morris and Douglass (**243**) have compared the lecture method with what they call the problem method in a course in the principles of economics. In the lecture method section the class work was done mostly by the instructor who lectured on the topics in the order given in the text. In the problem method class the course work was organized around a few major questions of contemporary interest. The assignments and readings for the two courses were identical. A long objective test was given at the end of the two terms of work. For the students equated on intelligence test scores those taught by the problem method showed a superiority of 11.4 per cent over the lecture method students. With the students equated on previous college records the superiority of those in the problem section was 22.2 per cent.

L. C. Pressey (**201**) conducted a class of 71 college students of elementary psychology in the effort to see if the formal lecture method could be avoided in so large a class. The class met five times a week, and on Monday a lecture was given introducing some new phase of the subject matter and preparing the students for the problems to be worked out during the week. For the next three days of the week

the class hour was a sort of laboratory period where the students worked out the problems suggested by the topic. On Friday the reports on the problems were handed in, and there was a general summary of the results of the week's work. The results of the course as measured by student opinion were distinctly favorable to the method used. The number of students in the experimental class who went on and took further nonrequired courses in the field was 25 per cent greater than in sections taught by the lecture method, and 15 per cent greater than in classes taught by a combination of the lecture and discussion methods.

Hullfish (**112**) tried a variation of the lecture method in teaching a class of 119 students in a course in principles of education. The material of the course was divided into units, the lectures were given at the beginning of the units. Then the students reported to a conference room in sections of 30, and worked on individual problems with the help and supervision of the instructor and his assistants. Here again student opinion was almost unanimous in approving the method of conducting the course.

Study Methods

Weinland (**252**) collected information from 20 college honor men and 5 men who were doing failing work concerning their study methods and he concludes that good students generally do the following things: work and study alone, avoid distractions, don't work on when tired but take short rest periods, work by assignments, are able to sustain and continue to work on uninteresting material, keep fairly complete notes, and cram when necessary. L. C. Pressey (**201**) studied 25 "A" and 25 "E" students from classes in psychology who were roughly paired as to year in college and I.Q. Examination of their habits of study revealed that the "A" students have regular habits of place and time of study, use the graphs, tables, and diagrams in their reading, and take notes on reading and lectures selectively and in outline form. "E" students have trouble expressing themselves and difficulties with sentence structure, and they sit up late for examinations trying to memorize the text. A remedial course including personal interviews, lectures on methods of study, a laboratory period for practicing these methods, and drill on especially weak points caused the group to improve greatly in college work as compared with a control group. Smeltzer (**218**) investigated by questionnaire what 21 college students thought were their chief difficulties. The following were among the most important: Instructors expecting too much

of the students, inability to concentrate while studying, instructors forgetting that the students don't always remember facts once learned, study time not used to advantage, and lack of ability to select the important material from a mass of information. It has been mentioned above that good students take fairly full notes.

Much of the work connected with high school and college courses is greatly dependent on reading assignments. Experimental studies have been undertaken to determine what sort of reading assignments bring the best results. Williams (259) conducted an experiment with six classes in ninth grade English. Three of the classes were taught by the intensive and the other three by the extensive reading method. At the end of the semester the results obtained by stenographic reports of the classes and pupil comments favored the extensive method. The pupils in the extensive reading classes had read many times more than those in the intensive reading classes. Moon (174) experimented with two groups of pupils who were equal in ability, and had them prepare their history lessons by two different methods. The class which read extensively and collected information as an incidental part of the process was able to remember more facts and do better on tests than the class which spent most of its time in intensive study of the facts to be remembered. When these same two groups of pupils both prepared their lessons by reading widely, they did equally well on the tests. Moon concludes that better results are obtained by the extensive reading method.

Good (85) made a study with two classes in a course in education. The two methods experimented with were the intensive method where the class read only the textbook material, and the extensive method where supplementary material was read besides the textbook. The two methods were alternated for the two classes after every two topics were completed. The first part of the objective tests used to test the classes was devoted to questions based on the textbook material, while the second part questioned the students on material treated more specifically in the supplementary reading than in the text. Results of the test indicate that the intensive group was always superior in the first part of the tests while the extensive group had the advantage in the second part. The intensive method was conducive to better ability in the reproduction of ideas, while the extensive method was superior in range and accuracy of information, problem-solving, and outlining. On the whole, Good feels that the extensive method is better than the intensive method.

L. C. Pressey (200) analyzed the textbooks in 16 representative

freshman college courses to determine the kinds of reading ability required by college courses. She gave 1,000 freshmen an examination including passages testing the chief kinds of reading skills most commonly found in the textbooks, and found the test useful in picking out students who would be likely to find difficulty with their college work. The main implications from her study seem to be that a small amount of training in certain skills connected with reading graphs, formuli, and problems, and some time spent on increasing reading speed would save many students from serious difficulty with their college courses.

S. L. Pressey, L. C. Pressey, and H. C. Martin (**201**) investigated the problem of the optimum reading load for university courses. They found experimentally that students could easily read 20 pages of reference material an hour allowing plenty of time for review and note-taking. They increased the reading load of a college course accordingly and further changed it by having a more complete check of the work with the result that the number of students in the course who later elected to take more courses in the same field more than doubled over the number doing this before the changes had been made in the course.

Examinations

Several investigations have been conducted which might be of help to the teacher who would like to know some of the commoner pitfalls to avoid in making out examinations.

Eells (**58**) made an experiment in which a group of teachers graded the same set of essay type examinations twice, the interval between the two gradings being eleven weeks. His results show that the correlation of the successive ratings of the same teacher on the same material averaged as low as .36. This would seem to suggest the inadvisability of using essay type examinations as the sole basis of grading students in a course.

Objective tests are likewise capable of showing low reliabilities. Lee (**127**) analyzed 60 objective examinations prepared by high school teachers and supervisors in order to discover the most frequently occurring faults of construction. He found that statements often failed to be clear because of involved phraseology, unnecessary phrases or clauses, and ambiguous wording. The directions for the test also often suffered from poor wording. Other faults were items the answers to which were obvious, trivial, unimportant items, repetition of items, statements which answered each other, lack of uni-

formity in the form of multiple-choice items, and bad provision for scoring the tests.

Brinkmeier and Ruch (24) studied 376 objective examinations made by high school teachers containing 10,756 questions in all to discover specific determiners of trueness and falseness. In the examinations they investigated they found the following to be true:

4 out of 5	statements	containing	*all* are false
3 out of 4	"	"	*always* or *never* are false
4 out of 5	"	"	*no, none, nothing* are false.
9 out of 10	"	"	*only* or *alone* are false
2 out of 3	"	"	clauses of cause or reason are false
4 out of 5	"	"	enumerations are true
2 out of 4	"	"	*may* expressing possibility are true

3 out of 4 statements containing such words as *most, some, often, generally,* etc., are true

Wiedemann (251) investigated the same subject using for his data objective examinations made by five college instructors at Columbia. His results are similar to those of Brinkmeier and Ruch given above. He concludes that makers of objective tests should be careful to make half the statements containing the specific determiners given above true and half false. Also they should avoid using more than one specific determiner of truth or falseness in one statement.

Brinkmeier (23) made another study of objective tests to see if sentence length was a specific determiner in true-false statements. She found that sentences of from 20 to 25 words in length tended to be true in 66 per cent of the cases and that sentences longer than 25 words were true in 80 per cent of the cases. This would spoil the value of the test if any of the pupils became aware of it. Jensen's (121) experiment on the relative effect of visual, oral, and visual-oral methods of presenting true-false examinations suggests that the method of presentation makes little difference in the scores although students usually preferred the visual method.

Experiments on the influence of examinations in promoting learning seem to disprove the attitude that examinations are only a necessary evil. Some experiments have been performed to determine the influence of the expectancy of an examination on learning. Jones (127) found no difference between the scores of a group which

had been warned that they would be tested on a lecture and a group which had received no warning, and he concludes that the attitude of expectation of an examination had no effect on learning.

Experiments on the effect of preëxamination indicate that this device can be made valuable. Jersild (**125**) investigated the comparative value of various types of preëxamination. Preëxaminations consisting of multiple-choice and essay type questions were an aid in furthering retention of the material to be learned except in the case of the true-false type of examination where it was of no help at all. Jersild also found that a question in the form of a direct interrogation as opposed to a narrative statement question stimulates the pupil to the greatest amount of activity and causes the largest amount of learning. Washburne's study (**249**) on the use of questions in social science material carries similar implications concerning the value of preëxamination. He found that when a group of junior high school pupils read a story or paragraph where the questions on the material preceded the material instead of following it, retention of the material was improved 40 per cent. The preview placement of questions resulted in a spread of generalizing ability to facts not covered by the questions calling for generalizations. Thus general comprehension of the significance of the facts is increased together with the number of facts recalled.

Two investigations have been made which attempt to measure the effect of examinations in motivating the pupils to study the material on which they are to be examined. S. L. Pressey and L. C. Pressey (**202**) conducted an experiment wherein they allowed the students in a college course in psychology to take a reëxamination if they were unsatisfied with their showing on any of the six half-hour tests given during the course. The score on the reëxamination was the only one that counted towards the student's grade. The experimenters found that this opportunity for a reëxamination motivated the students to more study and that more of the course material was learned than would have been the case if the opportunity had not been given. Pease (**190**) has made a study of the learning value of a period of cramming motivated by the announcement of an examination. Two groups of students taking the same course were paired on the basis of intelligent test scores. One group spent about 1½ hours in intensive cramming before taking the tests. This group did significantly better on the tests, and in delayed recall tests retained one-fourth to one-half of its advantage after six weeks, and one-eighth

of its advantage after twelve weeks. Pease also found that the value of cramming was in direct proportion to the intelligence of the person doing the cramming.

TRANSFER

In this discussion of the transfer of training in learning the main emphasis will be on experiments which measure the amount of transfer resulting from methods of teaching school subjects.

In the field of elementary school arithmetic Overman (187) compared the amounts of transfer resulting from three different methods of teaching addition. The method which produced the largest gain in ability to do the addition problems was one where the pupils were helped to formulate general methods of procedure from the specific examples taught, and where these generalizations about procedure were constantly emphasized throughout the teaching. Olander (185) experimented to find if a method of teaching simple addition and subtraction which included a few minutes every day devoted to generalization concerning the principles involved would have any effect on the pupils' ability to solve such problems. His results showed that the abstract generalization had no effect on the pupils' ability to solve problems. Mitchell (171) made a study to discover whether pupils who are able to solve specific arithmetic problems have formed a general conception of the principle involved which they could apply to solving any problems involving that particular principle. He made a list of specific problems and a list of general problems. Mitchell found that the pupils could solve more specific problems than general ones, and that the correlation of the scores on the two lists was only .52. He concludes that teachers should drill more on problems of a general nature, or that they should emphasize underlying principles while giving the pupils specific problems.

Ballew (5) has compared the effectiveness of laboratory exercises in high school zoölogy with and without drawings. The experiment was continued for 14 laboratory exercises and during each exercise one group of pupils made a drawing of the specimen under observation while the other spent the time answering the supplementary questions on the exercise. Objective tests were given after each exercise was completed which were designed to measure the pupil's memory of the structures of the specimens and also his analytical observations during the laboratory hour. Ballew found that the group which made no drawings answered more supplementary questions than did the other group. Also, as judged by the results of the tests, the construc-

tion of drawings seemed of no help in aiding the pupils to make analytical observations or to remember the observations made in the laboratory.

In a junior high school shop course including printing, woodwork, sheet metal, electricity, and mechanical drawing, Anderson (2) found that transfer from one of these activities to another was more apt to be negative than positive. A beneficial practice effect does take place to the extent that common factors appear in the different activities. These common factors may be similarity of tools, operations, materials, or products.

Woodrow (265) conducted an experiment to determine the effect of instructions in methods of memorizing together with some practice in memorizing poetry and learning nonsense syllables in columns of pairs. The effect of this type of training was compared with training which included only practice in memorizing poetry and learning nonsense syllables without any instructions in methods. Six tests were given at the beginning and end of the experiment, including tests of the memorization of poetry, prose, facts, Turkish-English vocabulary, historical dates, and a test on memory span for consonants. The per cent of gain on the final testing averaged 31 higher for the group which had been given instruction in principles and methods of memorizing than for the group which had merely practiced memorizing. In fact the undirected drill group showed no consistent gain on the tests while the group given directed drill showed a fairly large positive transference.

A course in educational psychology furnished material for an experiment on transfer by Watson (250). Four laws of learning were taught by means of illustrations from teaching situations, and by illustrations from nonhuman relationships to two sections of the same course. By giving a test designed to find the pupils' ability to analyze typical teaching and learning problems before the instruction in the laws of learning and again a week after this teaching, the ability of the pupils to apply what they had been taught was measured. It was found that the students who had been taught the laws in connection with teaching illustrations showed themselves unchanged after a week in ability to analyze teaching situations. But illustrations from teaching gave a very slightly greater transfer to ability to analyze other teaching situations than did animal or diagrammatic illustrations. Also the low I.Q. group gained more than the high I.Q. group in ability to analyze teaching situations.

Dorsey and Hopkins (54) provide evidence to the effect that if

students are given the suggestion of applying a certain kind of knowledge to a problem where that knowledge would be of help, they do better on the problem. By using the equivalent group technique they found this to be true where the application of methods of study to the study of a particular task was concerned, where knowledge of Latin could be used to define a list of Latin-derivative words, and where knowledge of descriptive geometry would help in solving a problem.

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COMMUNICATION

A Correction of Authorship

During the early spring months of this year there appeared from time to time in the daily press and in certain magazin n account of an investigation on sleep purported to have been made .he writer on students in Skidmore College.

The accounts are so misleading as to author: p that the writer in the interest of truth and of scientific accuracy begs the American Psychological Periodicals to publish the following note:

> The alleged findings of the beneficial effects of carbohydrates (notably as contained in Jack Frost candies! per radio report) upon the sleep of Skidmore students were made by Professor Donald A. Laird and his students at Colgate University. The data of the findings consisted of answers made largely by sophomores to a questionnaire devised at Colgate and claimed to have been scored, statistically treated, and interpreted by the same parties.
>
> The rôle played by the writer in the procedure was to receive and pass out the folders to an elementary class in psychology who filled them out the evening of the same day in their rooms, returned them to the writer's office the following day when they were at once shipped back to the Colgate authors.

Linus W. Kline.

Skidmore College, June 4, 1931.

NOTES AND NEWS

PROFESSOR DANIEL A. PRESCOTT of the Department of Education of Rutgers University has been appointed to the staff of the General Education Board for special studies in the field of secondary education. His work will continue through a year of leave from Rutgers University. During that time he will give special attention to the subjects of intellectual measurement and social studies as adapted to the needs of secondary education.

MR. EDMUND L. TINK, Director of Educational Research in the Public Schools of Montclair, New Jersey, is to spend the summer months in the service of the General Education Board on special studies in public school education.

ACCORDING to *Science*, Professor William McDougall, of Duke University, delivered the Ludwig Mond lecture at the University of Manchester on May 19.

THE Social Science Research Council announces that the closing date for making application for Research Fellowships in the Social Sciences is December 1, 1931, the awards to be announced not later than March 1, 1932. Further information and application blanks may be had by addressing the Fellowship Secretary, Social Science Research Council, 230 Park Avenue, New York City.

The basic stipend attached to these fellowships for a period of twelve months is $1,800 for a single, and $2,500 for a married Fellow, with adjustments upward in case there are dependents. Supplementary allowances are granted to cover travel and incidental expenses as needed.

The Research Fellowships are open to both men and women of American or Canadian nationality provided (1) they are not over thirty-five years of age, and (2) they are the holders of the Ph.D. degree or its equivalent in terms of other types of training and experience. In rare instances, the formal requirements stated above may be waived.

Dr. Winthrop Niles Kellogg, Associate Professor of Psychology, Indiana University; James Thomas Russell, Fellow in Psychology, University of Chicago, and Dr. Harold Dean Carter were among those named for awards of Research Fellowships in the Social Sciences for 1931–32 by the Social Science Research Council.

BOOKS RECEIVED

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